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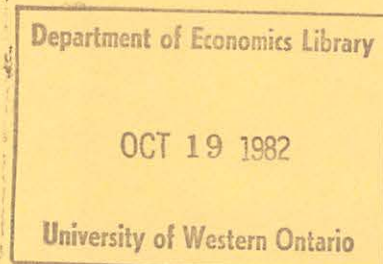
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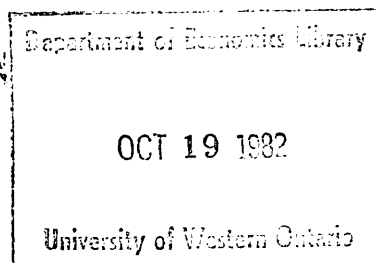
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DID MACROECONOMICS NEED THE RATIONAL EXPECTATIONS REVOLUTION?

by

David Laidler

A paper prepared for a conference on "Economic Policies for Canada in the 1980s" to be held in Winnipeg, Manitoba in October 1982 in honour of the 65th birthday of Clarence Barber. I am grateful to Peter Howitt and Michael Parkin for their comments on an earlier draft. Neither of them, however, is to be held responsible for the views I express in the following pages.



I

If any development in economic theory has attracted more attention in the last decade than the rational expectations hypothesis, it seems safe to say that macroeconomists at least have not heard about it. Some authors (e.g. Begg 1982) are now referring to a "rational expectations revolution" in the discipline and claims are being made on behalf of that revolution not unlike those which, once upon a time, were made on behalf of the Keynesian Revolution.

As its title suggests, this essay is devoted to assessing such claims from the standpoint of macroeconomics. This particular focus is important for two reasons. First, as Begg's readers are well aware, the rational expectations hypothesis has found, and continues to find, applications well beyond the conventional boundaries of macroeconomics per se, and I shall not be concerned, for example, with judging the significance of the hypothesis for the theory of asset market behaviour in this essay. Second, and of more importance perhaps, many of the more striking results associated with the "rational expectations revolution" in macroeconomics have involved not one hypothesis but two: the rational expectations hypothesis per se, and the proposition that the economy may usefully be modelled as if it were made up of a series of continuously clearing competitive markets. It is these two hypotheses which together form the core of what is often, somewhat misleadingly, called "New Classical" macroeconomics. I prefer the label "neo-Austrian" and shall use it in this essay.¹

The two above-mentioned hypotheses are logically independent of one another, as the following examples ought to convince the reader. In a deterministic world, the proposition that agents hold rational expectations

about the future time path of prices reduces to the assumption of perfect foresight about prices, and that assumption is present, albeit more often implicitly than explicitly, in virtually all the fixed price level IS-IM analysis that permeated the last generation of macroeconomics textbooks. At the same time some of Robert E. Lucas' pioneering work on inflation-unemployment interaction in a world of clearing markets (e.g., Lucas and Rapping 1969) utilized a mechanical error learning mechanism to model expectations in experiments which rendered expectations formed in that way systematically erroneous. Nevertheless, it is the combination of these two hypotheses which gives neo-Austrian macroeconomics its particular flavour. When I pose the question "did macroeconomics need the rational expectations revolution?" I am really asking whether or not this branch of our subject needs to be reconstructed root and branch on the basis of these two hypotheses. Since such neo-Austrians as Barro (1979), Lucas and Sargent (1981) seem to be arguing unequivocally that just such a reconstruction is necessary, I do not believe that I am erecting a straw man in posing the issue in this way.

There are many ways of stating the rational expectations hypothesis. At one extreme the phrase may signify nothing more than what Friedman and Schwartz (1982, p. 630) have called the "ancient idea" that, in making their decisions, agents will use all relevant and available information. Put in this way, the hypothesis is at best innocuous and at worst vacuous. All its content lies in the meaning one might attach to the words "relevant" and "available". The neo-Austrians attach very specific meanings to these words. To begin with they attribute to agents correct knowledge of the structure of the economy in which they operate. If such knowledge is complete and there is no inherently stochastic element to the economy's structure, then this

amounts to assuming perfect foresight, but if it is incomplete or if there is an inherently stochastic element to behaviour whose distribution is known, then it involves agents in making unbiased econometric forecasts of relevant variables. Furthermore, in arguing that, even though agents do not literally engage in sophisticated econometric forecasting exercises, they nevertheless behave "as if" they did so, the neo-Austrians gloss over the distinction between expectations and anticipations, and postulate that the information which they attribute to agents will be acted upon. Furthermore, for neo-Austrians the world of whose structure agents in question have so much knowledge is made up of clearing competitive markets.

As the reader will surely agree, when fleshed out in this way, the "ancient idea" of rational expectations is anything but vacuous, but it is also far from innocuous. Even if, for many people, the idea loses much of its a priori plausibility and appeal, this is not a reason to criticize it. Quite the contrary: to specify the hypothesis in this way is to deny the empirical relevance an enormous variety of a priori plausible and possible patterns of behaviour, and that makes the hypothesis a powerful scientific proposition. Whether it makes it a correct proposition in the sense that the empirical predictions which it yields are not refuted by available empirical evidence, however, is a different matter, and it is this issue of correctness that I shall mainly be discussing in the body of this paper.

II

Proponents of the "rational expectations revolution" argue that their approach to macroeconomics is not only superior to the "Keynesian" alternative on theoretical and empirical grounds, but also that it provides a sounder guide

to the analysis of macroeconomic policy. On the theoretical plane, the claim is one of superior microeconomic foundations, and I shall now briefly discuss this claim. Though I have grave doubts about it, they stem only partially from disagreement concerning the criteria that one might apply in forming judgements here. Economics seeks to explain a wide variety of phenomena, some of them micro and others macro in nature, and I would not deny that the fewer basic hypotheses the subject needs to cope with these phenomena the better. The argument that the hypothesis of the rational maximizing agent, be it a firm, a household, or a biological individual, is a basic element in economic analysis does not bother me either. If a body of theory dealing with issues which we usually classify as macroeconomic--the determination of the level of employment, prices, etc.--can be shown to be compatible with this basic hypothesis, then so much the better, and if it cannot, then so much the worse.

Now it is certainly true that the behaviour relationships proposed by Keynes in the General Theory (1936)--notably the consumption function--were presented by him as "laws" describing observable empirical regularities, rather than as consequences of individual maximizing behaviour. In its beginnings, macroeconomics did seem to lack firm micro foundations. However, long before anyone had heard the phrase "rational expectations", work was underway to counteract this defect. Whether or not we nowadays would approve of the details of particular studies, there can still be no denying that Friedman (1956,1957), Modigliani and his associates Brumberg and Ando (1954) (1963), Jorgenson (1967), Eisner and Strotz (1963), Baumol (1952) and Tobin (1958), to name a few important contributors, were all attempting to provide a basis in maximizing behaviour for relationships which, taken together, make up what we would usually recognize as a standard "Keynesian" IS-LM macro model; nor can there be any reasonable doubt that they largely succeeded in doing so.

There is, that is to say, no fundamental incompatibility between the component parts of the standard IS-LM model and the maximizing postulate. To this extent, it does not lack micro foundations.

There is, however, more to macroeconomics than isolated behaviour relationships. Any body of theory which deals with the economy as a whole must be concerned with the way in which individual agents, and groups thereof, interact with one another. It must have a foundation not just in the theory of individual maximizing behaviour, but also in the theory of markets. It is here that the microeconomic foundations of the two approaches to macroeconomics under discussion differ, and it is not hard to make the case that each one of them is, in its own way, unsatisfactory in this respect. The body of analysis pioneered by Patinkin (1965) Chapter 13, and Clower (1965) and brought to fruition, in various forms, by Leijonhufvud (1968), Barro and Grossman (1976) and Malinvaud (1977) was all directed to showing that, if prices are slow, relative to quantities, to respond to shifts in demand, then quantity changes, rather than price changes will play the role of equilibrating factors in markets generating Keynesian multiplier processes, which lead the economy towards "income constrained" positions of rest. In their turn, these positions of rest can be shown to be equilibrium positions of an IS-LM model.²

Thus there is a clearly specified market theoretic foundation for standard Keynesian macroeconomics. We did not need a "rational expectations revolution" because this was lacking. Rather, the proponents of its neo-Austrian form ask us to embrace their revolution because they regard that foundation as unsatisfactory, resting as it does on a postulate which directly contradicts the market theory to be found in most microeconomics textbooks. There it is price changes which are the equilibrating factors

in markets rather than quantity changes. If the flexible price general equilibrium model is the norm against which all other constructions are to be judged, then the sticky price postulate which underpins Keynesian macroeconomics certainly appears to be "ad hoc".³

On the other hand, I doubt if it would be difficult to find people to agree to the proposition that the assumption of complete price flexibility is also ad hoc. Certainly, if one is to make it, he must ignore the factors to which such economists as Keynes (1936), Hicks (1974), Tobin (1972) or Lipsey (1981) have pointed as providing a basis for price stickiness notably in the labour market, a basis, be it explicitly said, which explains such stickiness as the outcome of the rational maximizing behaviour of the agents operating in that market. According to this just mentioned body of work, the labour market does not conform to the competitive norm. Monopoly elements may be present, or, more fundamentally, externalities which stem from relative wages as well as their absolute level being an argument in individuals' utility functions. If factors such as these are admitted into the analysis, one cannot then develop a macro-theory by simple aggregation of individual experiments: instead interaction effects among agents become important. If the aim is to deduce macroeconomic predictions solely from propositions about individual behaviour, and that is, according to Lucas (1981), a key aim of his work, the competitive assumption must be maintained. His ignoring the micro-foundations of price stickiness is not therefore capricious, but is a necessary component of his research strategy.

Some theorists find a difficulty here, though. Frank Hahn (1982) in particular has argued that a model of competitive equilibrium with all markets always clearing is not the easiest in which to justify a role for

money. This might make a fastidious economist uneasy about using such a model as the basis for the analysis of the macroeconomic consequences of monetary disturbances. Of course it can be done, because money can always be introduced into such a model by assumption. Such a procedure however is yet again open to the charge of being ad hoc. Although attempts by such workers as Karekan and Wallace (1981) and Bryant and Wallace (1980) to find a foundation for monetary theory in the overlapping generations model of Samuelson (1958) represent an attempt to avoid this particular pitfall, whether these attempts will prove successful or not must be a moot point at the moment; though the arguments of Hahn (1982) and McCallum (1982) to the effect that the model in question does not capture money's essential role as a means of exchange, and hence cannot be used as a foundation for a theory of money, seem to me to be very powerful.

That well-known term of disapproval "ad hoc" has turned up three times in the last page or so. The lesson here is surely straightforward: the market-theoretic foundations of macro theories of all varieties are, in the current state of knowledge, shaky. We did not need the rational expectations revolution because the micro-foundations of existing macro-theory were non-existent or widely perceived to be hopelessly flawed, though they were, and remain, incomplete. However, although the micro-foundations of the new macro-theory are certainly different from those of its older rival, and are more appealing to anyone whose training in micro-theory has stressed the competitive Walrasian model, they too can fairly be termed incomplete. The question of whether or not they are "better" is not to be settled on a priori grounds. It seems to me to be an empirical matter.

Before I turn to empirical issues, a word should be said about the arguments that Lucas and his associates have advanced for the superiority on

theoretical grounds of the "rational expectations" notion per se over the mechanical extrapolation schemes that have so often been used to generate expectations variables in Keynesian models. Here I find little to argue about. It is of the very essence of macroeconomics that we need to understand the behaviour of the economy over time, and any macroeconomic model therefore needs a theory of expectations. Also, there is something very wrong with attempts to construct such a theory which arbitrarily assume that agents ignore information which is available to them, whose relevance they can perceive, and upon which they are able to act. To the extent that macroeconomists had to be reminded of these simple truths, and we did, we certainly needed the "rational expectations" revolution.⁴ The question, however, is whether, if we accept this part of the new doctrine, we also need to adopt what it offers us in the way of market theoretic foundations for macro-theory. It is to this, as I have already argued, empirical question that I now turn.

III

As we have seen, the micro foundations of the macroeconomics propounded by exponents of the rational expectations revolution are certainly different from those underpinning any "Keynesian" alternative. Though that gives us no reason in and of itself to prefer the newer doctrine, it does require us to take it seriously. Surely no one would argue with the proposition that it is healthy for macroeconomics that there be available alternative approaches to explaining the key variables with which it deals. If there do exist such alternatives, then their explanatory power ought to be compared wherever that is possible. From such comparisons we might expect to learn something both about our theories and about the world we live in.

It has been a frequent claim of the exponents of neo-Austrian macro-economics that the experience of the 1970s--particularly in the United States, though experience has been sufficiently similar elsewhere to suggest that the claim might be more broadly based--has constituted a crucial experiment in which Keynesian economics has been decisively refuted.⁵ The particular facts to which they have pointed are the co-existence of high, and indeed, on average, rising inflation with rising unemployment and slow and declining rates of economic growth. These facts, we are told, are not what would have been or were predicted by the macroeconomic orthodoxy prevailing at the beginning of the decade. Rather, high and rising inflation was expected to coincide with low and falling unemployment, and with more vigorous real growth.

The first thing to be said about this claim is that whether it seems true or not depends upon one's perception of just what constituted prevailing macroeconomic orthodoxy in the 1960s. To be fair to Lucas and his associates, they are explicit about this, and always refer to a style of macroeconomics, based on what Samuelson referred to as the "Neoclassical synthesis", which is exemplified by virtually all large scale U.S. macroeconometric models. Nevertheless, this particular style of analysis does not exhaust the Keynesian legacy. Those who, in the 1960s, believed that inflation was a cost-push phenomenon caused by real income growth failing to keep up with the rising aspirations of the labour force would undoubtedly have predicted that a slow-down of real growth, such as the seventies witnessed, should be accompanied by high and rising inflation; and they would have predicted that attempts to control that inflation by demand side policies would have led to rising unemployment.⁶

I do not refer to this particular brand of "Keynesian" economics because I am a sudden convert to it. I am not: I believe that certain other facts

generated by the 1970s make it hard to accept. Even so I draw attention to it in order to make the point that the evidence cited by Lucas and his associates refute only one, albeit once widely accepted, version of Keynesian macroeconomics, namely that in which price level and output behaviour are linked through a Phillips curve whose structure is such as to permit a permanent inverse inflation-unemployment tradeoff. Such a relationship was certainly believed to exist by many during the 1960s and 1970s. However it was not an essential feature of Keynesian macroeconomics; nor, crucially, is it a necessary implication of the price adjustment mechanisms postulated by the Neoclassical synthesis.

As Lucas (1981) has explicitly noted, perhaps the most careful exposition of that particular brand of macroeconomics is Patinkin's (1956) (1965) Money, Interest and Prices. One of that book's numerous virtues was the care which Patinkin took to remind his readers that many of the results he generated were conditional upon expected prices being equal to current prices. Given his purposes, there was no need for Patinkin to modify this assumption, but anyone seeking to use his work as a basis for analyzing the macroeconomics of inflation should have seen the need to do so. To put the same point in another way, when Lipsey (1960) set out the underlying micro-theory of the Phillips curve, using the same Samuelsonian price dynamics as did Patinkin, he should have recognized that the relevant price for his labour market analysis was not the money wage but the real wage. However he did not.

The upshot of this elementary but pervasive error was that, for a while, many economists analyzed the endogenous dynamics of inflation on the basis of an implicit assumption that all agents believe inflation to be an exogenous constant! However, and the point is not sufficiently

appreciated, this error was revealed and corrected by Phelps (1967) and Friedman (1968) before the 1970s generated any experiments, crucial or otherwise. No one who had read and accepted the basic thrust of those articles found anything in the 1970s to un-nerve him, nor did he need feel any uneasiness about adopting as the market theoretic basis of his macroeconomics the kind of analysis advanced, say, by Leijonhufvud (1967). What the 1970s experience did refute was the particular assumption implicit in far too much of the macroeconomics of the 1960s that money illusion can be a permanent phenomenon. That assumption should never have been a central characteristic of Keynesian economics, or of any other kind of economics for that matter.

Phelps and Friedman both made the basic point that inflation expectations would not remain constant during an inflationary episode, and in suggesting that those expectations would tend in fact to respond to experience, they also rendered existing orthodox models capable of generating that set of stylized facts known as "stagflation". However, in modelling expectations, the only nod that Phelps and Friedman made in the direction of any kind of rational behaviour was in imposing the requirement that an ongoing constant inflation rate would eventually become fully anticipated. In terms of the error learning scheme which they adopted, they insisted that the weights accorded to past inflation in forming expectations about the future sum to unity. Even this a priori requirement was too much for some exponents of the neoclassical synthesis, who subjected it to empirical test, found it apparently refuted, and so concluded that the long-run Phillips curve, though steeper than the short-run curve, still permitted an inverse inflation-unemployment tradeoff.⁷

Be that as it may, the claim of Lucas and his associates that early attempts at modelling expectations were mechanical, and their claim that

maximizing principles can usefully be applied in this area are amply justified, although, one should note that much of the work in which adaptive expectations were used abounds in informal warnings about taking that particular hypothesis too literally or seriously. It was often presented as no more than a convenient first approximation to the modelling of endogenous expectations, not suitable for use in all circumstances, and probably inadequate at times when policies were changing.⁸ But, and this is the crucial point, such warnings and qualifications did not impinge upon formal modelling exercises, and those who gave them showed no signs of appreciating that they were dealing with special cases of a general phenomenon which lent itself to formal modelling. To say, therefore, that those who used adaptive expectations did not take the hypothesis very seriously and recognized that there were many cases in which it was inadequate, is not to say that they understood the notion of rational expectations or appreciated its implications for macro modelling: they patently did not. Here we have a clear instance in which Economics undoubtedly needed one of the key ingredients of the rational expectations revolution.

IV

To say that agents will: use all information that is freely available to them; will take steps to acquire any other information for which the benefit outweighs the cost of acquisition; will act upon such information to the extent that they are free to do so; and hence will not, in a long run when they are free to act on all their information, make systematic errors, is not also to say that: the world behaves as if all markets are competitive and continuously clearing; all agents understand the workings and interaction of markets to the extent of being able correctly to forecast the outcome for the economy of any new exogenous shocks of whose nature they are aware; and that all real fluctuations are the result of random errors in forecasting exogenous variables. The former set of propositions is a very general statement of the notion of rational expectations to which any reasonable person might assent, and the latter is a very specific application of that general notion, hedged around with particular assumptions both about the nature of the economy, and about agents' knowledge of it, at which that same reasonable person might balk.

Nevertheless, as I have already noted above, it is the latter set of propositions which forms the basis of neo-Austrian macroeconomics, and it should require more than the observation that one particular alternatively grounded macroeconomic model has failed to cope with a particular set of stylised facts to persuade us of the desirability of embracing rational expectations revolution which it embodies. We need to know that there is no third or fourth option available among macro theories which can explain those same facts as well as the "revolutionary" model, or that, if there is, there exist other facts which enable us to reject those other options, before we can

reasonably be expected to embrace that revolution.

We have already seen that, if explaining the stylised facts of the stagflation of the 1970s is all that is required, third and fourth options are readily available, though, of course, the "revolutionary" model also can explain both stagflation and its cyclical character. Nevertheless we need to look at other facts if we are to make further progress in selecting the most satisfactory model from the menu now available. I have already referred to that particular offshoot of Keynesian economics in which money wage, and therefore price level behaviour is treated as a sociological cost push phenomenon, and noted that such a model has no difficulty coping with the broad outlines of the stagflation of the 1970s. Though this branch of macroeconomics is not central to the topic of this paper, it is worth pointing out that my reasons for rejecting it are not--I hope--ideological, but empirical. Space will not permit me to do more than assert those reasons: namely the silence of this approach in the face of the clearly cyclical nature of the time path of both inflation and output, and its apparent contradiction by the outcome of policy experiments such as that embodied in Mr. Anthony Barber's 1972 budget for the United Kingdom. Then an attempt to use fiscal and monetary stimuli to break through into sustained growth accompanied by lower inflation resulted instead in a short-lived real boom and a dramatic increase in the inflation rate. The Mitterand government in France seems recently to have provided us with a little more evidence of a similar character, this time uncontaminated by an oil price shock.

Be that as it may, let me now turn to the empirical evidence with which it is sufficiently difficult to reconcile the macroeconomics associated

with the "rational expectations revolution" as to persuade me to reject that doctrine. As I have argued in considerable detail elsewhere (Laidler 1982, Chs. 2-3) certain stylized facts generated by the voluminous literature on the demand for money function create difficulties for the rational expectations revolution, particularly those aspects of it whose crucial basis is the clearing competitive markets hypothesis. As we all know, the empirical work on the aggregate demand for money function which uses annual and quarterly data systematically demonstrates the need to postulate some kind of lag effect in the adjustment of actual cash balances to their long-run equilibrium level if conventionally acceptable criteria of goodness-of-fit and so forth are to be satisfied. It is also apparent that it is very difficult indeed to explain such lagged adjustment solely in terms of expectations formation, though this may well be part of the story.

The difficulty is that the kind of costs of portfolio adjustment which are usually used to justify such lagged adjustment effects would have no observable consequences in a model in which the supply and demand for money can be brought into equilibrium by the variation of a flexible price level. Price level changes cause real balances to vary for the individual agent without him encountering any adjustment costs. If the world really was made up of continuously clearing competitive markets, we would never observe anything but a long-run demand for money function (except for the consequences of expectation effects). On the other hand, if the price level is slow to adjust, then it is easy to show that the long-run short-run distinction will be important as far as the observed behaviour of the demand for money function is concerned.⁹ The facts I am citing are not quite fatal to the clearing markets rational expectations model, because sufficient ingenuity in manipulating distribution effects and expectations effects could reconcile

it with the evidence (see Laidler (1982, Ch. 3)). It is in difficulty however, in the face of stylized facts which are very easy to explain once price stickiness is postulated.

The basic facts of money-output-price interaction over the course of the business cycle also present problems. Neo-Austrian economics predicts that "anticipated" changes in the time path of the money supply, will affect only prices. "Unanticipated" changes on the other hand, though they too will cause prices to vary, will have output and employment effects as well as a result of individual agents misreading the signals being conveyed to them by prices. Thus, over the course of the cycle we might expect the consequences of changes in the monetary growth rate to manifest themselves predominantly in price level behaviour and only to a lesser extent in the time paths of real variables. Moreover because all quantity changes in a neo-Austrian model are responses to signals given by prices, we should also expect quantity changes to be contemporaneous with or perhaps even to lag a little behind, price changes.

There is surely no better established stylized fact than that, over the course of the cycle, the first effects of changes in monetary growth rates are concentrated upon quantities with their effects on prices coming through only later. Despite this, attempts have been made to show that the neo-Austrian model is consistent with observed price-output behaviour. Thus the results generated by Barro (1978) are widely cited in this context. However, for him, "anticipated" money is the forecast of a regression equation heavily weighted with lagged values of the money supply, and it therefore varies very little when the actual money supply changes. Current fluctuations in the money-growth rate are thus modelled as

being mainly unanticipated. Boschen and Grossman (1980) have argued that, because agents can read newspapers, contemporaneously published data on the money supply ought to form the basis of the anticipated money concept used to test neo-Austrian predictions. Anyone who finds this argument persuasive must regard Barro's anticipated money series as inappropriately constructed, and hence his evidence as suspect. Moreover, he must also agree that Boschen and Grossman's results, which show that money supply fluctuations that are reflected in contemporaneously published data nevertheless seem to cause changes in output and employment, while those that are not do not do so, weigh heavily against the neo-Austrian view of things.

Now with sufficient ingenuity in distinguishing between money supply changes which are observed but expected to be temporary and those which are observed and expected to be permanent, and in distinguishing between prices which are posted and those which are "really" charged, and so on, it would surely be possible to defend the neo-Austrian model against not only Boschen and Grossman's particular tests, but also against the general charge that it is incompatible with the stylized facts to which I have drawn attention. However, there is another problem arising from the nature of the demand for money function which ought to be raised at this point.¹⁰ It is commonly accepted that the expected inflation rate is an important component of the opportunity cost of holding real balances, and it has been well known, at least since the seminal work of Cagan (1956) that, ignoring real growth, when, in an inflationary environment, the rate of growth of the nominal money supply is cut, two consequences must follow if equilibrium is to be restored in the long run. First, the rate of inflation must fall, and second, the

quantity of real balances must rise, which is the same thing as saying that the ratio of the price level to the nominal money supply must fall. It follows that, on average, between the initial situation and the new long-run equilibrium, the inflation rate must be below the rate of monetary expansion. The question arises whether we can say anything more definite than this about the time path of prices, and the answer is that we can. However what we predict here is very different depending upon whether or not we postulate continuously clearing markets and rational expectations.

If agents' expectations are rational, in the sense that they understand the relationships between the money supply and the price level which I have just outlined; if they are aware of the change in the monetary expansion rate and expect it to persist; if all markets are to clear continuously; and if we insist on the economy ending up in equilibrium with a positive but finite stock of real balances; then the outcome of the experiment we are considering is well known. The price level will fall at once to a value compatible with the long-run equilibrium quantity of real balances determined by the new lower expected inflation rate and will thereafter rise at that rate, which is, of course, equal to the rate of monetary expansion.

Now I am not suggesting that anyone should take the above, very special, conceptual experiment literally, but it is nevertheless instructive. In particular, it reminds us that any anticipated change in the rate of monetary growth will have consequences not just for the ongoing inflation rate but also, if markets clear, should cause a step change in the price level. For a number of reasons, this is awkward for the advocates of the rational expectations hypothesis. To begin with, if the step change in the price level was itself anticipated, that variable would be taken, by market forces,

instantaneously to zero in the case of a cut in the monetary expansion rate or to infinity in the case of an increase. To insist, as they do, on confining the outcome of such experiments to situations in which a finite positive equilibrium demand for real balances exists, rules out the anticipation of step changes in the price level. However it does not also obviate the necessity for the step change in question to take place. If a new inflation rate is fully anticipated, the stock of real balances must change.¹¹ It goes almost without saying that, in the real world, we do not observe anything which remotely resembles such step changes in the price level, even in situations in which it can be asserted beyond any reasonable doubt that the rate of monetary expansion has been cut, is widely known to have been cut, and is widely expected to remain at its new lower level. The current situation in Canada is a clear case in point, as is that in both the United States and Britain. In this respect the outcome of the real-world experiments bears absolutely no resemblance to the predictions of the model advanced by advocates of the rational expectations revolution.

Sadly, the outcome of those experiments bears a great resemblance to the predictions of a model in which prices are sticky relative to expectations and expectations respond slowly to experience: in such a model the first consequence of tight money is lower output and the inflation rate falls only slowly. Once again, with sufficient ingenuity and sufficient hedging around of the basic model with special assumptions, one would probably be able to rescue the neo-Austrian approach. Indeed, the Liverpool econometric model of the United Kingdom in which monetary contraction leads to large increases in voluntary unemployment (see, e.g., Minford 1980) represents among other things, an attempt to mount

just such a rescue. Nevertheless we find that once again it is exceedingly awkward to defend the desirability of the rational expectations revolution in the face of evidence which gives the alternative theoretical framework no trouble at all.

V

It would be a short step from the foregoing comments about the relevance of evidence generated by recent policy experiments in Canada and elsewhere for neo-Austrian economics to the conclusion that the "rational expectations revolution" has made at best no contribution, and at worst a negative one, to our understanding of economic policy. However, it would be a great mistake to take such a step, because it is precisely in this area that the "revolution" has something of lasting importance to offer macroeconomics. As I have already repeated several times, neo-Austrian economics rests on two hypotheses, not one, and the empirical difficulties I have been discussing above arise more from the postulate of continuously clearing markets and its interaction with the notion of rational expectations than from the latter idea per se. As I shall now argue, the rational expectations postulate makes a key contribution to our understanding of policy problems quite independently of its association with the idea of clearing markets.

No result in neo-Austrian economics has attracted more attention than the Sargent-Wallace (1976) conclusion that a fully anticipated change in the time path of the nominal money supply will have no effect on real income but only on prices. At the same time, there is no result whose true significance has been more widely misunderstood. A fully anticipated change in money supply behaviour is first of all one that is known to be taking place, and

one whose long-run implications for the behaviour of equilibrium prices are fully understood by economic agents. However, there is more to it than that. If the change is to be anticipated, rather than just influence expectations, agents must be free to act instantaneously upon their newly acquired information, secure in the knowledge that their activities will be coordinated by markets in such a way that they will be able to fulfill their plans.

There is no reason to believe that a real world experiment, in which politicians announce a policy change, and then follow through on their announcement, ought to bear any close relationship to the conceptual experiment of Sargent and Wallace. Why should agents believe politicians? Why should they expect the policy change to be sustained? What evidence is there to suggest that they understand the quantity theory of money? How many of them are free to act on new information even if they wish to do so? and so on? The policy invariance proposition does not have to be read as a proposition about the real world, however. Rather, it may be read as an eye-catching counterexample to a pervasive approach to the analysis of economic policy which has, often unwittingly, taken for granted certain things that had no business being taken for granted.¹²

The approach in question is, in essence, an application of the standard analysis of the individual maximizing agent, the agent under study being the policy maker who is endowed with, or entrusted with the care of, a social utility function whose arguments are various policy targets. He is pictured as having control of certain policy instruments which he manipulates in order to maximize the utility function in question, subject to a set of constraints given by the structure of the economy with which he is concerned. The matter of the design of economic policy then naturally breaks down into three steps. First, the social utility function must be discovered or devised;

second, quantitative information about the structure of the economy must be sought; and third, a straightforward mechanical exercise in the application of the principles of constrained maximization yields the right settings for the policy instruments.

I shall touch briefly on the thorny questions surrounding the concept of a social utility function in a moment, but the neo-Austrian critique of this approach to policy analysis concentrates on the nature of the structure of the economy and the appropriateness of treating it as an unvarying constraint on the conduct of policy. Sargent and Wallace's analysis, and, of course that of Lucas (1976) remind us of the elementary fact that the "structure" of the economy is nothing more than the outcome of the systematic behaviour of individual agents. Each one of them is engaged in exactly the same type of utility maximizing exercise as the policy maker, subject to a constraint, however, part of whose structure is the systematic behaviour of the policy maker himself, not to mention the reactions of other agents to the conduct of policy. Thus, as the conduct of policy varies, so in general will the "structure" of the economy. An approach to the analysis of policy which ignores this proposition is, according to Lucas and his associates, hopelessly flawed. The Sargent and Wallace policy invariance theorem may be regarded as a vivid illustration of this flaw, even if it is not treated as a serious prediction about how the real world works.

The most important implication of the neo-Austrian insight into the nature of economic policy as far as macroeconomics is concerned is that it provides a much stronger and more general basis than previously existed for the case against discretionary policy. Thus Friedman (1960) rested his case for a monetary rule on the existence of long and variable time lags in the transmission mechanism of policy whose nature and structure were ill-understood. Implicit in his argument was the possibility that, with the passage of time and

the growth of knowledge, discretionary policy might come to be more effective than a rule. The neo-Austrian insight considerably weakens this particular line of reasoning by warning us that knowledge of the economy's structure, generated under one policy regime, may not be relevant under a new one. Moreover it bases this warning not on a general worry that in human affairs, things might after all be different tomorrow from what they are today, but on a very precise reason for believing that this might be the case. The possibility of isolating those aspects of the economy's structure which do not depend upon expectations remains, so advances in knowledge may still enable policies to be improved. However the scope for such improvement now seems much smaller, and the difficulty of attaining it much greater, than they did two decades ago.

Of course the neo-Austrian insight leaves at least as many questions open as it answers. Changes in the policy regime are far from being the only source of disturbance to the structure of the economy. Thus the often heard admonition to adopt a policy rule, and a simple one at that, does not really solve many problems. For example, it may or may not be wise to stick to a $k\%$ money growth rule while the private sector is learning to adapt its behaviour to a change in the relative price of energy, but to settle this issue, one would have to know about how the economy's behaviour would change while it was absorbing information about a change in energy prices and in the conduct of policy. A modelling exercise which imposes the rational expectations hypothesis, and perhaps that of clearing markets too, is to some extent useful in coping with such questions, inasmuch as it tells us--if we take stability on faith--where the economy is likely to end up. However, it tells us nothing at all about the time path towards this ultimate solution. If agents take time to learn, and markets take time to clear, this is an important omission. However,

if the rational expectations revolution has not solved the questions of how agents learn about changes in their environment and about how the economy behaves while they are learning, it has nevertheless made a significant contribution simply by enabling us to formulate those questions.¹³

There is another area in the analysis of economic policy which has been as much overlooked by the neo-Austrians as by those whom they have criticized. It is an obvious fact that policy makers and the general public do not simply communicate with one another through behavioural signals given anonymously in the marketplace. They are in constant touch with one another through the political process as well. Private sector agents do vote, and they do organize into lobbies which try to influence the conduct of policy in ways which will favour them. Politicians do seek votes, and do seek to influence public opinion. Public sector bureaucracies do have interests of their own to protect. Such matters have not been neglected by macroeconomists--see, for example, Johnson (1971) or Meltzer and Richards (1981)--but they have not yet been integrated into the neo-Austrian analysis, which is still concerned more with the technical question of how to maximize a social utility function than with understanding how the political process copes--the word is borrowed from Gordon (1980)--with the continuous and conflicting pressures out of which a consistent social utility function may or may not arise in the first place.

Nevertheless, it is unfair to criticize people who have been working hard at the analysis of one genuine and difficult problem for failing to get to grips at the same time with another. As I noted at the outset of this section, the contribution of the "rational expectations revolution" to policy analysis has lain in deepening our understanding of the nature of the constraints that the structure of the economy places upon the conduct of

policy and its contribution there has been a real one. Moreover, though proponents of the revolution have linked the notion of clearing markets to that of rational expectations in their writings on policy matters, if one treats what they have had to say as a series of counterexamples to a prevailing orthodoxy, rather than as a series of predictions about how policy will actually work out in practice, then their contribution in this area is one which should be taken notice of by anyone concerned with the analysis of policy, regardless of his views on the appropriateness of assuming clearing markets.

VI

The reader will by now realize that I do not think that there is any straightforward and unequivocal answer to the question "Does macroeconomics need the rational expectations revolution?" From the point of view of pure theory, there can be no doubt that this revolution embodies the most successful attempt that we have seen to link microeconomics to macroeconomics, though the novelty lies not in finding a basis in individual maximizing behaviour for the components of conventional macroeconomics, but in showing that a model made up of clearing competitive markets can reproduce at least some of the main characteristics of that complex of macroeconomic phenomena known as the business cycle. To the extent that any branch of any subject is healthier when it is forced to entertain more than one explanation of the behaviour with which it deals, macroeconomics certainly benefitted from the "rational expectations revolution".

However, I would stress here that the revolution did indeed add one more alternative explanatory framework to the corpus of the discipline. Though its proponents have sometimes given the impression of believing that their approach alone could deal with the stylized facts of the 1970s, in rather the same way

that Keynes produced a unique explanation of the 1920s and 30s, this simply is not the case. The brand of economics which could not cope with the facts of stagflation, and it certainly existed, was, as I have argued, one which treated money illusion as a permanent phenomenon. There always were, and remain, a number of alternative approaches to macroeconomic analysis which do not suffer from this fault, and neo-Austrian economics is only one of them. I have also argued that when confronted with certain stylized facts--the presence of lag effects in empirical aggregate demand functions, the time path of prices and output in the wake of monetary changes, and so on--the neo-Austrian approach does not fare too well. I have no doubt that given sufficient ingenuity, its proponents can cope with these objections, just as those who insist that the earth is the centre of the universe can explain all observations in terms of epicycles, but I have also pointed to the relative ease with which models which incorporate an hypothesis of price stickiness can cope with the same facts.

From the empirical point of view, then, except in the not unimportant sense that it has generated good questions, I am inclined to argue that macroeconomics did not need the rational expectations revolution at least in its neo-Austrian form. However, as I pointed out at the very beginning of this paper, that form of the revolution rests on two hypotheses and not one: rational expectations and clearing markets. As I hope is clear from the arguments advanced in the course of this essay, the source of the empirical difficulties is overwhelmingly the second of these. That is why, despite my doubts about the empirical content of the "revolution" I nevertheless judge it to have made an important contribution to policy analysis. In that context, the role of the clearing markets postulate

is to facilitate the construction of vivid and telling counterexamples to a prevailing orthodoxy. Their real force comes from the rational expectations notion itself, and hence they are important far beyond the boundaries of the particular conceptual experiments which generate them. In short, my conclusion is that macroeconomics did, and does, need the rational expectations hypothesis, but that it does not need the rational expectations revolution. We need to absorb the implications of that hypothesis into our already existing macroeconomic framework, rather than throw the latter overboard in favour of neo-Austrian theory.

FOOTNOTES

¹I have explained this choice of label elsewhere--Laidler (1982)--in terms of the methodological individualism and reliance on the competitive general equilibrium model which characterizes this body of work. The reader should note that there is now emerging a body of analysis which, in combining the notions of rational expectations with overlapping contracts, rather than clearing markets, promises to provide an alternative to the neo-Austrian approach. See, e.g., Fischer (1977), Taylor (1979). For a textbook treatment, see Parkin (1982) Ch. 25.

²On this matter, see Barro and Grossman (1976), Ch. 3 .

³It is a frequent theme in the writings of Lucas (e.g. 1981) that Keynesians invalidly criticize his models because their predictions fail to conform, not to observed behaviour, but to the predictions of Keynesian models. In this complaint he is quite justified, but the tendency displayed by neo-Austrians to characterize any postulate about price behaviour that departs from the competitive norm as ad hoc, also sometimes comes perilously close to criticizing someone else's model for failure to conform to the conventions of one's own.

⁴Anyone who believes that those who used the adaptive expectations hypothesis in their work nevertheless fully appreciated its limitations in this regard should consider the accelerationist hypothesis of Friedman and Phelps. This hypothesis has agents forming expectations about inflation in a manner which, in the long run, is appropriate only for a situation of constant inflation in an experiment whose outcome involves ever increasing inflation. I am grateful to Don Patinkin for this striking example of the slowness with which our profession grasps the full implications of new ideas.

⁵ This claim is stated with no equivocation by Lucas and Sargent (1981). However, in a later paper, Lucas (1981) is much more tentative in advancing arguments along these lines.

⁶ Indeed, Clarence Barber and his associate John McCallum (1980) have advanced just such an interpretation of Canadian experience in the 1970s and have cited just such evidence as I here mention in support of their position.

⁷ See for example Robert Solow (1968).

⁸ My own work of the early 1970s comes to mind as an illustration of this. See Laidler (1975), Chs. 3 and 10. Nevertheless, the ifs and buts that there permeate my verbal discussion find no counterpart in my formal modelling.

⁹ The alert reader will note that interest rate flexibility could do as well as price level flexibility in making adjustment costs irrelevant in the aggregate demand for money function. This point suggests that we understand a good deal less about the role of money in the aggregate economy than we thought we did, given the manifest importance of the long-run, short-run distinction in empirical work. On these issues see Laidler (1982) Ch. 2.

¹⁰ The following passage draws heavily on discussions with Michael Parkin, who is not, however, to be implicated in the conclusions which I draw.

¹¹ The problem under discussion here was well known to those working in the area of money and economic growth in the 1960s and 1970s, not least to Sargent and Wallace (1973) who explicitly assumed that step changes in the price level are unanticipated. Robert Barro (1978) sidesteps the issues involved here in his empirical study of the roles of anticipated vs. unanticipated

changes in the monetary growth rate in influencing prices and output by inappropriately treating the nominal interest rate as an exogenous variable.

¹²Sargent and Wallace's own attitude to their analysis is ambivalent. The first part of their paper presents their experiment as an example of the power of the rational expectations idea to generate novel predictions, but the closing pages of the same paper invite the reader to regard their analytic results as empirically relevant.

¹³The reader should note that Lucas and Sargent's (1981) account of criticisms of the neo-Austrian approach shows that they are well aware of these issues.

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